Reviewer’s report

Title: The effectiveness and efficiency of diabetes screening in Ontario, Canada: A population-based cohort study.

Version: 1 Date: 18 March 2010

Reviewer: Edward W Gregg

Reviewer’s report:

General Comment:
This is one of the more thorough examinations of screening, testing, and incidence to date. Given the perpetual dilemma about how to screen, test, and among whom, to prevent and control diabetes, this is potentially an important contribution. The analyses appear are thoughtfully constructed and appear sound. The main weakness of the paper, however, is a lack of clarity in some key areas that prevent us from really understanding the results and drawing conclusions. Specific ways that this paper could be improved include the following:

1. A minor point. The 2nd and 3rd sentences of the introduction seem a bit out of place. Although they are true, it’s not quite clear how they relate to this specific analyses.

2. Page 4, last sentence. The use of the term “anonymised” is not quite clear here. Does this mean that the data from administrative data specific to the individual are linked to the health surveys. Or alternatively, is the health survey sample simply linked to the aggregate administrative data for those that are in the health survey?

3. Page 5, 1st paragraph. How was diabetes status defined using the Ontario database.

4. What proportion of all diabetes testing may be expected to be conducted and coded as a “SBG” test?

5. Page 6, last paragraph to page 7, 1st paragraph. The approach to define undiagnosed diabetes is confusing. Could the authors give a bit more clarification of how this is derived and how it has been validated? What sources and magnitude of error is expected around this manner of estimating undiagnosed diabetes?

6. The Figures need considerably more notation as part of the legends, such that the reader can interpret them without digging back through the methods section. For example, what is the decile of risk based on? Figure 1 just states “decile of 5-year diabetes risk” whereas Figure 2 states “estimated by DPoRT”. Are they both estimated by DPoRT?

7. Perhaps more importantly, the results section neglects to present some of the basic information that will be of greatest interest to the reader. (Some of this
information is in the appendices, but would need synthesis and description to integrate into the paper) These questions include:

a. What is the 5 year incidence among people who are not tested initially? I realize this may not be possible to know if the incidence measure depends upon the same variable as the testing variable. But we can’t really discern this from the methods at present.

b. What is the relationship between frequency of testing and subsequent risk?

c. How does the NNS vary according to sub-groups? Page 8 describes this to some degree, but it would be nice to see more thorough presentation of this.

8. Finally, the discussion would benefit from a bit more comment on the implications for screening and testing policies. Among whom should we be screening and testing more, and among whom less? Presumably the NNS is affected by the specificity of the test and by the prevalence in the population. There could be more comment here on how these factors influence (or don’t influence the study findings).

**Level of interest:** An article of importance in its field

**Quality of written English:** Acceptable

**Statistical review:** No, the manuscript does not need to be seen by a statistician.

**Declaration of competing interests:**

I declare that I have no competing interests.